direction of motion of the satellite nucleus that in this case its motion around M will be accelerated by its collision with m. . . . The effect of the accelerations by the scattered material is to enlarge the orbit of the satellite nucleus, and to prevent its being drawn down upon the growing planetary nucleus.

Now the speeds of the larger planets and of their satellites are as follows:

	Speed in Miles per Second	
	Of Primary in Orbit	Of Satellite about Primary
Jupiter	8.1	
Sat. 1		10.7
2		8.5
3		6.7
4		5.1
Saturn	6.0	
Sat. 1		9.0
2		8.2
3		7.9
4		6.3
5		5.3
6		3.5
8		2.0
Uranus	4.2	
Sat. 1		3.5
2		2.9
3		2.3
4		2.0
Neptune	3.4	
Sat. 1		2.7

On the very face of the table it will be seen that six satellites contradict the book. When we get into it deeper we find they all do. Thus if we suppose the colliding particles to be equally distributed in space we have for those within the planet's orbit:

$$\int_{\frac{1}{2}}^{1} \frac{(2a-1)^{\frac{1}{2}} a^{\frac{1}{2}} da}{\int_{\frac{1}{2}}^{1} a\,da}$$

for their mean velocity at the point of collision; a being the semi-major axis of any particle.

This equals 0.79 of the planet's orbital speed. A result substantially similar is got for any other possible distribution.

From this it appears that all the large satellites of all the large planets have spatial speeds which would cause them to be retarded by such impacts or exactly the opposite of what the book states. So that the supposed proof by this of the planetesimal hypothesis turns out to be a disproof of it.

From what we have said it will be seen that the hypothesis expounded will not work.

Percival Lowell

THE NOMENCLATURE QUESTION

To the Editor of Science: May I add a few words to the excellent letters by Mr. F. N. Balch[1] and Dr. W. H. Dall?[2]

It is necessary first to assume that zoologists in general accept or wish to accept the rules drawn up by the Nomenclature Committee of the International Zoological Congress. The assumption may be a ridiculous one, but it will at any rate be admitted that until those rules are generally accepted further discussion is premature.

I agree with Dr. Dall that most cases can be settled by a rigid application of the code. There are a few in which the interpretation or application of the code may be obscure. These must be remedied either by greater precision in the rules or by the decisions of a court in the manner described by Mr. Balch. There are other cases in which the consequences of the rules are perfectly clear, but at the same time exceedingly unfortunate—so unfortunate indeed are some of them that a great many zoologists are beginning to say "So much the worse for the rules."[3] A phrase has often been used that we should accept the principle of priority "tempered with common sense." This would be all very well if there were such a thing as common sense, but it is notorious that in these matters *quot homines, tot sententiæ*. In a recent paper[4] I have therefore ventured to repeat an old proposal, for which the time now seems to be more ripe, and as that paper may not be very widely seen, I ask you to print the following extracts:

[1] Science, June 25, pp. 998–1000.

[2] Science, July 30, pp. 147–149.

[3] See, for instance, a letter to *Nature* for August 27, 1908, pp. 394–395, signed by many leading British zoologists.

[4] "Some Common Crinoid Names, and the Fixation of Nomenclature," *Ann. Mag. Nat. Hist.* (8), IV., pp. 37–42, July, 1909.

The only possible alternative to strict following of rules is that zoologists should agree to accept as final the decision of some authority by them appointed. The vehicle for such authority already exists in the Nomenclature Committee of the International Zoological Congress, the only body that has any claim to represent either all branches of zoology or all nationalities.

If I may indicate a convenient form of procedure, I would suggest that those zoologists who wish to protect certain names should lay the complete facts of the case before the committee, and should accompany their request for the retention of certain definite names in defiance of the rules by the signatures of as many workers on the group affected as they can obtain. Due announcement of the proposed step should be made in certain widely circulated journals and a reasonable time should be allowed for the reception of protests. The committee should ultimately give its decision, and this decision should be published in the aforesaid journals. A summary of the labors of the committee in this direction would of course be given from time to time in the publications of the International Zoological Congress.

The precise style or mode of appointment of the desired authority does not greatly matter, if only zoologists will agree to accept it. But that it should consist of experts will doubtless be conceded. The ruling may be arbitrary, but it must none the less be made with knowledge of all the circumstances of the case and of the results that will follow from it. It must be clearly understood that the decision is to be made, not because it is in accordance with the rules, but because it is to produce practical convenience.

The next steps appear to be, on the one hand, to find out whether a sufficient number of leading zoologists are in favor of these proposals; on the other hand, to induce the International Committee to undertake this added responsibility.

F. A. Bather

SCIENTIFIC BOOKS

Die binokularen Instrumente, Nach Quellen bearbeitet. Von Moritz von Rohr. Berlin, Verlag von Julius Springer. Pp. 223, 1908.

This book has been written by one of a small group of men who have grown into prominence by their original work in connection with the optical establishment of Carl Zeiss at Jena, where for many years the scientific head was Professor E. Abbe. This firm has been known the world over for its high standards; and in photographic and microscopical optics, regarded from both the practical and the purely scientific standpoint, Abbe up to the time of his death was without a peer. His successors, Czapski, Pulfrich and von Rohr have adopted the ideals of their master; and in addition to the details involved in directing the scientific work of a large business they have found time to write books that are accepted as important contributions to optical science.

The first part, or theoretic section, of the present volume includes a general introduction, a chapter on monocular vision, and one on binocular vision, in which account is taken of certain limitations that must be heeded, due to the fact that the eye is not a simple instrument but an optical system which differs in some important respects from artificial instruments. This is true, whether the vision is direct or indirect, with one eye or with a pair of eyes used in conjunction with each other.

The greater part of the book is taken up with the historic development of the subject. The earliest binocular instrument dates back to the beginning of the seventeenth century when Lipperhey, in Holland, constructed the first telescope, and gave to Galileo the starting point for his epoch-making discoveries in astronomy. Lipperhey soon constructed a double telescope consisting of a pair of parallel tubes, each with convex and concave lens, so that by simultaneous use of both eyes double as much light could be received from the same distant object. There was no conception that the images received were in any way different, but the binocular instrument which we call an opera glass, was made prior to 1625, even though not much used. Before the end of that century improvements had been introduced for adaptation to varying interocular distance, and for focusing to suit the varying distances of objects.

Aside from the use of the telescope the superiority of a pair of eyes over a single eye